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ABSTRACT

This paper contains informal remarks on action research in social psychology from its post World War II origins to its current status. Kurt Lewin first described action research in the 1946 article, "Action Research and Minority Problems," as a three-step process of program planning, program execution, and follow-up evaluation. Ronald Lippitt and Marion Radke also described 10 aspects of action research in 1946, based on programs to reduce anti-Semitism and racial prejudice by the Commission on Community Interrelations of the American Jewish Congress and the Institution of Ethnic Affairs. Participation in the research by the research subjects was the most important element. By 1948 four additional approaches to action research developed: diagnostic, participant, empirical, and experimental. Questions and comments on the current status of action research have been raised by Nevitt Sanford, Leona Tyler, and Stuart Oskamp. What happened to action research--whether it died, moved into other disciplines, or reemerged with new names like program evaluation or social policy research--remains unanswered. (BS)

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ACTION RESEARCH: ITS ORIGINS AND EARLY APPLICATION

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ACTION RESEARCH: ITS ORIGINS AND EARLY APPLICATION

Action research as a concept originated shortly after World War II. While I played a part in its early history, I am by no means confident that I can give either a comprehensive or an entirely accurate account of its development. Hence, I hope you will take what I have to say, not as historical truth, but as my personal interpretation of what happened.

As I proceed, I shall be speaking on some occasions about action research as a concept and a label, and, on other occasions, about action research as a type, or types, of research activity. As you will see, the concept has been defined in more than one way, and more than one type of research has attracted the label.

The origin of action research is generally attributed to Kurt Lewin. His first description of it appeared in 1946, and was called "Action Research and Minority Problems." It was published in volume two of the Journal of Social Issues. In that article, Lewin gave two very general definitions of action research. One was "comparative research on conditions and effects of various forms of social action." The other, quite different, was "research leading to social action." Both definitions were given in the context of the even more general category of research intended to "improve social management or social engineering." Lewin went on to give a more specific, three-step definition. This involved, first, developing an action program, which he referred to as "planning;" then, second, carrying out the first step in this program, which he called "execution;" and, finally, reconnaissance, or factfinding, regarding program results. There then begins a new three-step cycle. Using what has been learned from the factfinding, those responsible for the program and the research would carry out a second cycle of planning, execution, and factfinding about results.

Lewin's second description of action research occurred in a chapter in the initial 1947 edition of SPSSI's Readings in Social Psychology, edited by Newcomb and Hartley. It was called "Group Decision and Social Change," and was devoted primarily to discussing participation in group decision-making as a social management approach to changing group conduct. In his chapter, Lewin repeated his definition of action research as a three-step process of program planning, program execution, and follow-up evaluation or factfinding.

This conception of using research as a regularized, routine step in the building of more effective action programs is what Lewin meant by integrating research and action. There are several early indicators of his interest in such an integration. The earliest occurred while he was still in Berlin. In 1919 and 1920 he had published two papers on the idea of making agriculture and industry more efficient by research on the tools and operating procedures used by workers. Later, in 1939, after he had been in the United States for six years, Alfred Marrow involved Lewin in a series of consultations on productivity problems in a manufacturing plant. Marrow was one of the Americans who had spent time with Lewin at the University of Berlin and in 1933 was instrumental in helping him with the move to America. After completing his doctorate in psychology at New York University, Marrow elected to take over from his father the ownership and management of a substantial textile manufacturing concern. When his company faced the competitive strains of the 1930s depression years, he turned to psychological research as a tool for dealing with the questions of productivity on which his firm's existence depended. With the advice and continued involvement of Lewin, he began a series of experiments on group decision by employees regarding their operating procedures. A number of these experiments were done by Lewin's student, Alex Bavelas. The frequently cited study of group decision-making by Coch and

French was a part of the series. It may be that this pattern of incorporating research into an industrial production program served as a model for Lewin's later discussion of action research in relation to intergroup relations programs.

In both his 1946 and 1947 articles, Lewin used yet another study to illustrate action research. In this case, the party concerned about program effectiveness was the Interracial Commission of the State of Connecticut. The Commission, whose task was to foster better intergroup relations in communities throughout the state, contacted Lewin early in 1946. By that time, he was no longer at the University of Iowa. He had established the Research Center for Group Dynamics at MIT, as well as a research division of the American Jewish Congress called the Commission on Community Interrelations. The function of the latter was to apply research to problems of prejudice and discrimination. I was its research director.

The experiment that was planned with the Connecticut Interracial Commission had as its purpose a test both of training workshop methods and of post-training social support. The first hypothesis was that potential leaders would function more effectively if trained by experiential, role-playing procedures. The second was that persons who had been trained to function later as mutual support teams would be more effective than those trained to operate as single, independent individuals. Ronald Lippitt, Leland Bradford and Kenneth Benné were in charge of the training. The impact of the workshop training on back-home effectiveness was assessed by interviews of both the trainees and persons who observed them at work. Overall effectiveness was found to have increased substantially, and this was particularly true for trainees belonging to the mutual support teams. Lippitt describes the results in his 1949 book called Training in Community Relations.

However, the Connecticut training workshop is most famous for an entirely serendipitous discovery. This is the way it came about. The research staff had a practice of gathering each evening to report on data collected during the day. During the evening, most of the trainees went home to their families. But since those who remained had little to do except sit around, they asked if they might attend the staff meetings. Although most of the staff feared the consequences of discussing a participant's behavior in the presence of the individual, permission to attend was granted nevertheless.

On the first night after one of the research observers had reported something about the group behavior of one of the three trainees who were sitting in, that individual broke in to offer an alternative interpretation of what had happened in the group, and the reasons for it. The discussion that followed between the trainee and the observer, as well as the group's trainer, was apparently quite lively. In his book, Lippett says that Lewin recognized that the interchange might provide an additional source of data, and decided to continue the practice of open staff meetings. Grapevine reports the next day brought out another group of trainees that evening. Soon, most of them were attending, and staff-trainee discussions about interpretations of the trainees group behavior became a lively part of the evening staff meetings. Eventually, trainee groups began convening their own evening meetings, at which they appraised their group behavior during the day.

This serendipitous discovery, as you may know, gave birth to the sensitivity training movement and its many offspring. But, in addition, and of more relevance to my topic, it brought a new emphasis to action research, an emphasis on immediate feedback of data to research subjects. Such feedback may not only provide new data, as Lewin recognized, but also may become an influence for behavior change, as it did in Connecticut. Such change, in

turn, may change the researcher's focus of inquiry. Some later advocates of action research have made this immediate feedback of data to research subjects its principle feature. It is often spoken of as making the research subjects collaborators or partners in the research enterprise.

I began by saying that the origin of action research is generally attributed to Kurt Lewin. However, there are contenders for this distinction. In 1946, Ronald Lippitt and Marion Radke described action research in an article in the Annals of the American Academy of Political and Social Science. They trace the origin of action research as a label to two brochures announcing the research and action programs of two recently activated research institutes. One of these was the Commission on Community Interrelations of the American Jewish Congress. The other was the Institute of Ethnic Affairs. Lewin, you recall, was a key figure in establishing the Commission. Lippitt was on the board of directors of the Institute.

According to Lippitt and Radke, action research involves ten aspects. The most distinctive of these are participation by the research subjects in identifying the knowledge they need from research, in deciding on the sample of persons to be studied, in data collection, and in interpreting data. Clearly, the emphasis is on the action-motivating effects of having research subjects participate in the research at every stage. Lippitt and Radke illustrate action research by reference to eight research reports. All appeared in the period 1941 to 1945. It turns out that Lippitt himself had been involved in several of these studies while assistant director of the research staff of the Boy Scouts of America.

Of special interest is the fact that the earliest of the eight studies cited by Lippitt and Radke was published by the University of Michigan Adult Education Program and was titled "Centerville Studies Itself." Its author was

Alvin Zander, who later became a Lewin student at Iowa and then his faculty associate in the Research Center for Group Dynamics at MIT. Lippitt, who was another of Lewin's students and faculty associates, received his early training in group work. These facts suggest intriguing historical questions. Might action research as a type of research have originated in the fields of group work and adult education? Might it have been taken into the Lewin circle of students and researchers by Lippitt and Zander? Might it there have encountered its industrial counterpart in the past experience of Lewin and two other of his students and research associates, Alex Bavelas and J. R. P. French, Jr.? And, if so, might action research, as a label, have originated not so much as a new research orientation, but as a term to facilitate communication between researchers and action-oriented lay people?

The next steps in the conceptualization of action research occurred at the Commission on Interrelations of the American Jewish Congress, the second of the research institutes that Lewin founded. The commission staff found itself responding to a wide variety of questions about programs to reduce anti-Semitism and racial prejudice. Sometimes the questions could be attacked with field experiments. Sometimes they involved motivating people to act, and, when this was the case, the participation and feedback principles that had emerged from the Connecticut workshop experiment were incorporated in the research. However, it became clear that not all program and policy questions could be answered in these ways, and this led to the description of additional approaches to action research. These were described in two articles, both published in 1948, by Isidor Chein, John Harding, and myself. One, called "The Field of Action Research," appeared in the American Psychologist. The other, called "The Use of Research in Social Therapy," was published in Human Relations. The basic ideas in these papers had been presented earlier, at

scientific meetings in the summers of 1946 and 1947. The latter occasion was a SPSSI symposium held 37 years ago.

The position taken in these articles was that there are four varieties of action research. They were designated as (1) diagnostic, (2) participant, (3) empirical, and (4) experimental. I shall give a brief description and illustration of each.

Diagnostic action research takes its name from the fact that the research activity is focused on fact finding and interpretation. Out of such fact finding may grow a recognition of a need for remedies as well as ideas of the direction which remedies should take. Diagnostic action research will typically be done at the request of leaders in a situation in which problems are being experienced. The chances of the research leading to change depend heavily on the resources and power of these leaders. A successful example was presented by James Worthy at a 1952 symposium on action research organized by the Industrial Division of APA. Worthy was then in the personnel department of Sears, Roebuck. He described a continuing series of surveys of employees that concentrated on the satisfactions and frustrations they experienced on the job. The Sears managerial hierarchy understood that it was their task to put into operation remedial suggestions emerging from the surveys. Succeeding surveys gave feedback that might reflect the success of the remedies put into effect.

By contrast, an early study of the Commission on Community Interrelations made clear the action limitations of diagnostic action research. In this case, the request for research guidance came from community leaders. There had been a series of incidents between racial and religious groups in the community in question. These were capped by a minor riot in front of a synagogue on one of the Jewish High Holy Days. The commission staff

interviewed samples of each of the major ethnic groups in the community, as well as indigenous and institutional leaders. The findings pointed clearly to multiple sources of frustration, irritation, and hopelessness among the youth of the community. These included factors such as seriously crowded housing and the absense of recreational facilities. Unhappily, the community leaders had neither the resources nor the power to correct such matters. Recommendations that proposed potential palliatives such as community organization to facilitate interreligious and interracial communication drew little support. Their superficiality was probably all too evident.

The second variety of action research, labeled participant action research, takes either of two closely related forms. One form, illustrated by the Connecticut workshop experiment, stresses immediate feedback of data to the research participants. The second form stresses the involvement of research subjects in research planning and data collection. An early example was the use of research to assist the human relations committee of a city to carry out its function of reducing discriminatory practices and increasing constructive intergroup activities. A starting point in this research was the assumption that most people in the city were unaware of the types of discrimination being practiced and where it was occurring. A further assumption was that if a body of representative citizens collected the evidence about discriminatory practices, they and others in the community would be more likely to support corrective actions. These assumptions led to a process that began with a fact finding survey. Plans for the survey were developed by ethnically heterogeneous citizen working committees in various areas of community life, for example, housing and employment. These committees were provided technical guidance by social scientist consultants. Other citizens were enlisted for data collection. After the technical

consultants summarized the data, the citizen committees participated in interpreting the data and developing recommendations for action. During this phase, the social scientists contributed their experience about what usually works best. The citizen committees also took responsibility for reporting to the community and for follow-ups on the implementation of their recommendations. This process came to be called the community self-survey. Two members of the Commission on Community Interrelations staff, Margot Wormser and Claire Selltiz, published guidelines for doing self-surveys in a book called How to Conduct a Community Self-Survey of Civil Rights.

The third type of action research was called "empirical." Empirical action research begins with thinking about the shortcomings of a program in theoretical terms, as, for example, Lewin did in arriving at his hypotheses about group decision and group commitment in motivating workers and changing food habits. The next step is to try out some change in the program. A record is kept of what is done and what happened. Account is taken not only of the workings of the program as changed, but also of other events taking place that may influence the program's effects. Effects on the program participants are examined as systematically as possible. As experience accumulates, it is used to guide subsequent program changes.

Ideally, the researcher following this pattern works with the same agency or institution for a period of time. Or, if not, with a succession of similar ones. While it is obviously possible to be mislead by such case history research, sophisticated professionals have often used it to develop valid principles. Examples of both misguided and valid outcomes are frequent in clinical medicine.

The fourth variety of action research, the experimental, differs from the third only in what Lewin called the reconnaisance, or fact finding phase. The

assessment of program effects takes experimental or quasi-experimental form. As I noted earlier, this was Lewin's preference. He and his associates used it whenever possible. The Commission on Community Interrelations employed it in a series of studies of how to reply to anti-Semitic remarks, and in studies of the race relations effects of desegregated housing. Early action researchers discovered the difficulties of maintaining experimental controls in an action program carried out by a collaborating agency--or, in the case of quasi-experiments employing contrasting programs--of locating equivalent program settings. Nevertheless, in hierarchical organizations such as secondary schools, government agencies, business and industry, and defense agencies, such settings could often be found, and, on the whole, such organizations proved receptive to experimentation.

The rest of my remarks have to do not with the history of action research, but with present day discussions of it. In 1970, Nevitt Sanford titled his Kurt Lewin Memorial Award address "Whatever Happened to Action Research?" He answered in two ways. First, he concluded that perhaps it "never really got off the ground," meaning that it was never widely influential in psychology or social science. Second, he said that if one looks outside the main streams of psychology and social science, action research is still very much alive. The places to look, he noted, are in the fields of public health, social welfare, criminology, community psychology and program evaluation research. To illustrate action research--or research-action, as he prefers to call it--Sanford described his own studies of higher educational institutions. These studies stress the interaction between interviewer and interviewee, much as had the immediate feedback form of earlier participant action research. Later, in his 1980 Henry Murray Award address, Sanford described similar action research carried out by Royal Foote

in a food processing plant. This work included not only interviews by the researcher of persons at all levels in the plant, but also periodic interviews of employees by their supervisors. Since the latter interviews represent a form of data collection by the research subjects themselves, Foote's study reflects another of the characteristics of participant action reearch: apparently, the study had a substantial impact on productivity in the plant. According to Sanford, officers of the parent firm reported that the plant where Foote did his research had become the leading producer among ten similar plants located elsewhere in the country.

Leona Tyler, one of APA's past presidents, is another psychologist who has raised questions about the current status of action research. In her 1983 book, called Thinking Creatively, she says that, after the 1940s and 1950s, the importance of action research declined almost to the zero point. She believes it has come into prominence again, in part as the consequence of questions about the social relevance of psychology. She gives few details, but does credit action research with being the forerunner of a current research approach called "new paradigm research." This approach emphasizes collaboration between researcher and research participant.

A third current description of action research has been provided by Stuart Oskamp in his 1984 text called Applied Social Psychology. Oskamp gives as his major illustration a remarkable 20-year research program carried out by George Fairweather. The purpose of Fairweather's research was to facilitate the community adjustment of mentally ill patients during the period in which they were being prepared for discharge from mental hospitals. A key component of the work was the use of halfway houses that provided experience in group living, self government, and outside employment. After what Fairweather calls the innovative phase, in which the researchers gradually improved the

effectiveness of these halfway houses in preventing recidivism, a nationwide, carefully controlled dissemination experiment was conducted. Its purpose was to discover the most effective ways to induce the 263 mental hospitals in the nation to develop their own halfway houses. Clearcut results were obtained. It was found that the inducement process had to involve not only detailed guidelines for halfway houses, but the presence of an expert consultant.

Was what Fairweather did action research? It certainly dealt with a practical problem. Moreover, it studied the impact of a program change. Also, during the dissemination phase it involved the collaboration of the staff of 25 hospitals in trying to develop their own halfway houses. In the innovation phase, the repeated trials of halfway house procedures certainly fit Lewin's three-step description of planning, execution, and fact finding followed by replanning, new execution and additional fact finding.

But in his dissemination phase, Fairweather speaks of inducing mental hospitals to adopt the new procedures. Only in the participant variety of action research has the question of inducing or motivating action been thought to be a problem. Ordinarily, the integration of research into ongoing action programs has been counted upon to eliminate the need to sell program changes once their value has been established. Fairweather himself calls his work experimental social innovation, and is quite clear about the distinction between the innovative and dissemination phases. I note this because it seems to me that current work on social issues by other social scientists also takes this form.

To conclude, I am afraid I can only repeat the question Nevitt Sanford asked 14 years ago: What has happened to action research? Did it never really get underway? Did it live only briefly during the 1940s and 1950s, and then die? Did it siesta for a decade or two, during the heyday of laboratory

social psychology, only to re-emerge, incognito, under such pseudonyms as program evaluation, experimental social innovation, and social policy research? An amateur historian is not the one to answer such a question. Perhaps, someday, a true historian will tell us.